Response to Reviewer#1

The responses to the reviewers' comments are highlighted in blue, and the revised text is *italicized*.

Interactive reviewer comment on the manuscript *Coupling the TKE-ACM2 Planetary Boundary Layer Scheme with the Building Effect Parameterization Model*

GMD-2024-205

By Zhang et al.

General considerations

In this contribution the authors present the coupling approach of an urban 'building effect parameterization' (i.e., the surface exchange parameterization) to a recently proposed (improved) boundary layer parameterization scheme for atmospheric RANS type models. The new coupled scheme is compared to a version with a bulk surface exchange treatment, and to the results from another PBL parameterization (one of the 'standard schemes in the literature), once with the bulk surface exchange and once with the 'building effect parameterization'. The approach is evaluated on two case study scenarios with an idealized surface characterization (regular cubes) and an LES (PALM-4U) as a reference. Then, a month-long simulation for the Pearl River Delta (China) with a number of mega-cities (including Hong-Kong) is performed. Data from three lidars (wind profiles), and 31 surface stations (urban and rural) are used for validation.

The study is well designed, and pretty well described (the 'plan' for the paper is good, and much of what needs to be known, can be found somewhere). I cannot say, however, that it is well written (I have added quite a number of 'detailed comments' – mostly related to language or formulations, etc.). A serious language/style update by a native speaker would certainly greatly improve the value of the paper.

Even if I have labelled one of the comments as 'major', I think its resolution is straight forward – so that I can recommend the paper to be published subject to minor modifications.

Major comments

1. Real case simulations & data: the presentation of the data is not complete. The lidars, when introduced should be characterized (urban, rural) and some basic information

on vertical resolution and accuracy should be provided. Also, for the surface stations, information should be provided on the explicit meaning of the different LCZ classes ('compact high rise, LCZ1, etc.), and how many are available for each type (how many urban, how many non-urban), what 'G class' (e.g., Fig. 17, 18, ..) means. Much of this can be found somewhere (I can, for example add the different numbers in each panel in Fig. 17) but the authors could support the reader in providing this information. Furthermore, Figs. 14-16 have 10 urban classes, plus 'water cells' plus 'natural cells', while Figs. 17-19 have 7 urban classes (the remaining three are probably not available) plus 'G stations' and 'rural stations': how do the latter translate to the water cells and natural cells? I suggest to add a sub-section in Section 2 with some of this information.

Dear Reviewer:

Thank you for your thorough review and valuable feedback. We appreciate your positive remarks regarding the design and structure of our study. We acknowledge the need for improvements in language and style, and we are committed to addressing these issues. We have responded to your detailed comments to enhance clarity and coherence. Additionally, we have appointed a native English speaker from AsiaEdit (https://asiaedit.com/) to assist with a comprehensive language and stylistic revision. We are grateful for your constructive input and are pleased that you find the main issues straightforward to resolve.

Firstly, we have provided explanations for the Local Climate Zones (LCZ) codes 1 to 10 and A to G in Table B1. In the revised manuscript, we maintain consistency by avoiding quick switches between terms such as 'LCZ G' and 'water surface', as well as 'natural' and 'rural' by using 'water surface' exclusively to refer to 'LCZ G' and 'rural land' for other non-urban land types.

Furthermore, we have introduced the characterization of wind speed LiDAR units in Section 2.4.1, where essential details, including resolution, accuracy, and operating frequency, are described. Additionally, we clarify that the land cover type for each LiDAR unit is determined by the LCZ classification of the nearest model grid. For instance, the King's Park LiDAR is located within an LCZ 1 grid, and is therefore abbreviated as KP_LCZ1.

Similarly, we have added a brief introduction to the surface stations data obtained from the Global Telecommunication System. The characterization of these surface stations is also informed by the LCZ code of the nearest model grid. Furthermore, we have explicitly listed the number of available stations for each LCZ classification, as detailed in Table B1.

Table B1. Local Climate Zones (LCZ) classification scheme.

LCZ code	Built type	Number of available surface stations	LCZ code	Land cover	Number of available surface stations
LCZ 1	Compact high-rise	2	LCZ A	Dense trees	4
LCZ 2	Compact mid-rise	1	LCZ B	Scattered trees	0
LCZ 3	Compact low-rise	0	LCZ C	Bush and scrub	3
LCZ 4	Open high-rise	3	LCZ D	Low plants	0
LCZ 5	Open mid-rise	1	LCZ E	Bare rock or paved	0
LCZ 6	Open low-rise	2	LCZ F	Bare soil or sand	1
LCZ 7	Lightweight low-rise	0	LCZ G	Water surface	10
LCZ 8	Large low-rise	3			
LCZ 9	Sparsely built	0			
LCZ 10	Heavy industry	1			
	Subtotal of urban stations	**13**		**Subtotal of non-urban stations**	**18**

Minor comments

l.48 '…they work with few…': maybe better 'they have only been coupled to a few … (I think they would also work with all the other schemes – btu it has not been done)

Thank you for highlighting this point. Line 46 has been revised to:

'However, multi-layer BEP/BEP+BEM models are adopted less widely than the Bulk scheme or SLUCM because they have only been tentatively coupled to a few planetary boundary layer (PBL) schemes [e.g., Boulac (Bougeault and Lacarrere, 1989), MYJ (Janjic, 1994), and YSU (Hong et al., 2006) added recently by Hendricks et al. (2020)]. This is primarily due to ´ the challenges associated with incorporating the transformation of mean kinetic energy into TKE within a first-order closure PBL scheme, such as the YSU scheme. '

l.60 'have shown that the TKE-ACM2….'

Thank you for the careful evaluation of the manuscript. We have revised the wording in line 69 accordingly as:

'They showed that the TKE-ACM2 outperformed two other operational PBL schemes, Boulac (Bougeault and Lacarrere, 1989) and ACM2 (Pleim, 2007b), in simulating the vertical profiles of wind speeds.'

l.62 'at *the* urban station….': this suggests that the reader knows which urban station is meant. Please rephrase.

We have rephrased the sentence in line 71 as:

'However, overestimated wind speeds persisted throughout the entire surface layer at stations classified as urban type, probably due to the discrepancy resulting from the Bulk parameterization of surface layer fluxes.'

l.72 '...from *the* high-resolution lidar': same as before (this suggests that the lidar had been introduced before). Reformulate to '....from a high-resolution lidar'.

'...from the high-resolution LiDAR' has been reformulated to:

'*... from a network of high-resolution wind speed LiDAR units*'

l.87 Energy conserving

Revised accordingly.

l.84 '...K is the eddy viscosity....'. Do I have to assume that K is equal for all 'zeta' (l. 91). If not (what would be better supported by the literature) , K should also get an index zeta.

We understand that this expression may cause confusion to readers who might assume K is the same for scalar and momentum. We have revised the expression in the equation for K such that $K_\zeta = K_h$ if $\zeta \in [\theta, q]$ and $K_\zeta = K_m$ if $\zeta \in [u, v]$. Eqns.2 and 3 are revised accordingly.

$$\overline{w'\zeta_I'} = -K_{\zeta,I}\frac{S_I(\zeta_{i+1}^n - \zeta_d^n)}{V_i \Delta z_I} + \mathrm{Mu}(h - z_I)(\zeta_1^n - \zeta_i^n) \quad \text{Eqn.2}$$

$$\frac{\zeta_i^{n+1} - \zeta_i^n}{\Delta t} = \underbrace{f_{\text{com}}\,\mathrm{Mu}\zeta_1^n}_{\text{upward cowvective traspport}} - \underbrace{f_{\text{conv}}\,\mathrm{Md}_i C_i^n + f_{\text{conv}}\,\mathrm{Md}_{i+1}\zeta_{i+1}^n \frac{\Delta z_{i+1}}{\Delta z_i}}_{\text{donwand tranport}}$$

$$+ \underbrace{(1 - f_{\text{coev}})\frac{1}{V_i \Delta z_i}\left[S_I \frac{K_{\zeta,I}(\zeta_{i+1}^n - \zeta_i^n)}{\Delta z_I} - S_{I-1}\frac{K_{\zeta,I-1}(\zeta_i^n - \zeta_{i-1}^n)}{\Delta z_{I-1}}\right]}_{\text{local tramport}} + \underbrace{\frac{F_i}{\Delta z_i}}_{\text{env. forcing}} \quad \text{Eqn.3}$$

$\mathrm{K_h}$ and $\mathrm{K_m}$ is likely to differ in their magnitudes, especially when there are convective thermals. The evidence is given in very detail in Li (2019) which is properly cited. $\mathrm{K_m}$ is related to $\mathrm{K_h}$ by $\mathrm{K_h} = \mathrm{K_m}/\mathrm{PR_t}$ where $\mathrm{PR_t}$ is the turbulent Prandtl number. The parameterization of Prandtl number adopted in this study is consistent with that in TKE-ACM2 (Zhang et al., 2024) which is the Businger's relationship (Businger et al., 1971).

The text has been revised in line 130 to:

'*The eddy diffusivity is equal in magnitude for scalars (Kh) and TKE (Ke) and is related to eddy viscosity (Km) through the turbulent Prandtl number (Prt), which is a key parameter pertinent to heat transfer (Li, 2019):*

$$K_e = K_h = K_m / Pr_t$$

where $K_m = C_K l_k e^{1/2}, C_K$ *is a* $\mathcal{O}(1)$ *empirical constant, the parameterization of* Pr_t *is consistent with that in Zhang et al. (2024) which follows Businger et al. (1971), and …*'

l.115 'C_eps is an empirical constant and l_eps corresponds to….'

Line 122 is revised as:

'… $\epsilon = \rho C_\epsilon e^{3/2} / l_\epsilon$ *represents the TKE dissipation rate where* C_ϵ *= 1/1.4 is an empirical constant and lϵ corresponds to the characteristic length of energy-containing eddies.*'

l.162 uniformly distributed in the vertical: this may be a good idea in a CBL but how about the near surface?

When a multi-layer UCM is turned on in WRF, e.g., BEP, the interpolation of prognostic variables (ζ) below the first half eta level such as U_{10}, T_2 still follows M-O similarity theory (MOST) as if in Bulk simulations, despite it is doubtful that MOST is better justified in the roughness sublayer than in the urban canopy layer. The deviation of MOST and explicitly resolving the height below $1\Delta z = 12.5\ \mathrm{m}$ is not discussed in this study. However, according to Shin & Dudhia (2016), a vertical resolution $\leq 20\ \mathrm{m}$ is deemed as a high resolution in a mesoscale model configuration. The comparison between LES and WRF+BEP is made by linearly interpolating the finer LES grids to the coarser WRF+BEP grids.

In the real case simulations of the present study and in fact in many other studies, e.g., Bhautmage et al. (2022), Shen et al. (2019), the first half eta level corresponds to roughly 9-13 m, which maintains a reasonable balance between the computational cost and accuracy in the urban canopy layer.

l.164 '…one corresponding to a moderately…'

Revised accordingly.

Fig. 2, caption: the different types of lidars should be referenced (UTSS, HT, KP), and briefly explained (possibly in the text) what their strengths weaknesses are.

The subsection (Section 2.4.1) is revised to describe details of instrumentation of LiDAR units deployed at different sites in Hong Kong including USTSS_LCZ5, HT_rural, and KP_LCZ1.

l.215 '...it is found that quasi-....'

Revised accordingly.

l.215 '....when LES...': how is the time for having reached quasi-equilibrium diagnosed?

Shin & Dudhia (2016) found that the TKE increases in time until 1 hr and stops growing after that, supporting that LES has reached a quasi-equilibrium state. In their study, the time scale 1hr corresponds to approximately 6τ where $\tau = PBLH/w_*$ is the large eddy turnover time. Similarly, a factor of 5 is found in other literature, e.g., Ayotte et al. (1996), Pleim (2007). In this study, the time series of TKE exhibits a similar trend to aforementioned studies, where TKE reaches a maximum, followed by a slight descend, and stops growing after that. The maximum vertical velocity shows a similar trend. Ultimately, we found that a time scale corresponding to approximately 10.2τ shown by the vertical dotted lines in the time series could be a critical value for determining LES has reached the quasi-equilibrium. After 10.2τ, although the time series exhibits fluctuations in the magnitudes, the instantaneous value does not show considerable deviation from the mean. There is no particular algorithm for diagnosing this time scale. Instead, due to the inability to store LES output at each time step, we found that domain averaged profiles after spinning up approximately 10.2τ=6,300 s and 4,200 s in two cases would be appropriate to drive the WRF simulations.

l.223 usually called 'turbulent fluxes'. However, it would be better to delete 'outputted' - these are just the 'turbulent fluxes from PALM'.

Thanks for your careful proofreading. We have corrected 'turbulence fluxes' to '*turbulent fluxes*'. Also, we have deleted the word 'outputted' from the sentence.

l.224 very often, what we can see in a figure has been plotted... (so, the verb 'to plot' is somewhat obsolete in this context). May be '....schemes are contrasted in ...'.

Thanks for the suggestion. We have revised the sentence in line 238 to:

'*The horizontally averaged u and θ profiles during the last 6τ are displayed in Fig.4 and the turbulent fluxes from PALM and computed from WRF PBL schemes are contrasted in Fig.5.*'

Fig. 6, caption: please add for which case this RMSE is determined and what 'the truth' is (assumed to be).

We have revised the caption of Fig.6 to:

'*a-d): RMSE for $\overline{w'\theta'}$, $\overline{w'u'}$, θ and u calculated below the PBL height for Case 10WC by taking the LES results as the ground truth, respectively; e-h): same as a-d) but for Case 24SC*'

Fig. 6: I would find it 'more convincing' if the black dots would be displayed as a 'dotted line' (and not as a dot at each level) – then it would not appear as a black line in the lower parts of the panels......

Thanks for the suggestion. I assume you mean Fig.4 (u and theta comparison) and Fig.5 (turbulent fluxes comparison) but not Fig.6 as Fig.6 is a bar plot and has no dot plots. We have now revised Fig.4 and Fig.5 such that the black dots representing LES results are connected through solid lines.

1. 231 'a smaller warm bias' would possibly sound better

Revised accordingly.

l..234 'becomes stable....': this is indeed a feature of the CBL. Some authors have even defined a 'neutral level', i.e. the height where slightly unstable transits into slightly stable (formally, there might even be such a height in the LES)

Thanks for the comments.

l.241 '....within *[the]* UCL and near *[the*]PBL height where the relatively constant w'θ' in the middle UCL is not exhibited [reproduced?] in either BEP simulation.'. Here, I think this is a little 'underselling' the BEP simulations. They at least to some degree reproduce a strong deviation in the profile at canopy height (the two others cannot reproduce this), the relax in the middle of the CBL (and yes, the vertical gradient is too small).....

Thanks for pointing this out. We have rephrased line 265 to:

'Greater discrepancies in the magnitude of $\overline{w'\theta'}$ were observed in TKE-ACM2+BEP within the UCL and near the PBL, height where the relatively constant $\overline{w'\theta'}$ in the mid-UCL was not reproduced in either BEP simulation; however, the drastic reduction in $\overline{w'\theta'}$ at roof level was well captured, indicating that the physical interaction with buildings was reasonably considered.'

l. 249 'This has shown the wind shear at the roof level is underestimated...': I am not sure what the authors want to say with this. Maybe this is just a matter of wording? – 'thus it appears that the BEP parameterization results in an underestimation of wind shear at roof level, when compared to the LES'.

Indeed, we aimed to convey a plain fact. We have rephrased line 254 to:

'It should be highlighted in Fig.4b that from the ground level to the top of the UCL, both BEP simulations overestimated the wind speed in contrast with an underestimation in the mixed

layer. It thus appears that the BEP parameterization resulted in an underestimation of wind shear at roof level when compared with the LES.'

l. 250 'it is discovered…': first of all I suggest to start a new paragraph. Second, momentum flux decreases (increases in magnitude…) with height. Third, 'at some height' (as it appears in the LES) seems to be some 2-4 canopy heights (in b) and d), respectively). Fourth, this cannot be called 'discovered' here – this was even one of the reasons for the development of the BEP scheme (i.e., that it had been discovered earlier, that momentum flux was not constant with height in urban canopies).

Thanks for the detailed comments. First, in the revised manuscript, we have started a new paragraph. Second, we corrected the momentum flux decreases from the ground (not increase). Third, we have substituted 'at some height' to 'at approximately 2 to 4 times the canopy height'. Fourth, we rephrased the sentence to 'It is observed that …'. Consequently, the whole sentence in line 271 is revised to:

'*The momentum flux decreases from zero at the ground level to a maximum value at approximately 2 to 4 times the canopy height followed by a descending trend in BEP simulations, in contrast to the monotonically descending trend in simulations when the Bulk method was adopted as shown in Fig.5a*.'

l.260 'similar behavior of the two schemes is found…'

Revised accordingly.

l.275 I think the dashed line is blue in Fig. 5d

Revised accordingly. This typo was due to that we changed the color scheme for all plots suggested by the journal editor to allow readers with color vision deficiencies to correctly interpret the results.

l.279 top of the RSL, rather

Revised accordingly.

Fig 7 I suggest to repeat the definition of delta_U (i.e., BEP-Bulk) in the caption. Same in Fig. 8 for theta

Agreed. As suggested by Reviewer#2 we improved Figs.7, 8, 14, 15, and 16 by contrasting TKE-ACM2 minus Boulac, both with and without BEP. In the caption, we have repeated the definition of $\Delta U(TKE\text{-}ACM2 - Boulac)$ in the captions.

l.300 beginning a new sentence: Figure 8…..

Revised accordingly.

Fig. 9, caption: delete 'plots the' ; 'at USTSS, HT and KP': are these locations? I recall too have seen different symbols in Fig. 2 – and thought this to be different types of instruments. I suggest to add an ultra-short sub-section in Section 2, describing the instrument type, vertical resolution and some accuracy statements from the manufacturer.

The words 'plots the' have been deleted.

'USTSS, HT, and KP' are indeed LiDAR units located at different locations.

Section 2.4.1 is revised to contain the abovementioned information:

'*2.4.1 Landuse data and wind LiDAR observation network*

This study adopted the 17-class LCZ classification scheme (Demuzere et al., 2022) to more accurately capture the highly variable urban morphology within the domain of interest. The distribution of LCZ 1 to 10 (urban) grids and LCZ A to G (non-urban) grids is depicted in Fig.2c. Each class is defined in Table B1.

A wind speed Doppler LiDAR network (see Fig.2d) has been operational in Hong Kong since March 2020, continuously monitoring wind conditions and playing a crucial role in validating regional downscaling results. The network comprises three WindCube 100S LiDAR units manufactured by Vaisala. Each unit measures the vertical profile of the wind speed at an elevation angle of 90◦ . The units measure 25-m intervals starting from 50m above ground level, with an accuracy of <0.5m/s for wind speed and 2 ◦ for wind direction. Although each LiDAR outputs data at a frequency of 1Hz, measurements are averaged hourly and archived due to storage limitations. We represent the land cover type of each LiDAR unit using the LCZ classification associated with the nearest model grid following Ribeiro et al. (2021).

The LiDAR unit at the Hong Kong University of Science and Technology Supersite (USTSS_LCZ5) is located on the east coast of Kowloon Island, where the nearest model grid center falls within classified as LCZ 5 (open mid-rise). The second LiDAR, installed on the southeastern peninsula of Hong Kong Island (Hok Tsui), is surrounded by natural vegetation and referred to as HT_rural. Lastly, the LiDAR at King's Park in downtown Kowloon, where the average building height is 60m (Kwok et al., 2020), is located within an LCZ 1 model grid (compact high-rise), and designated as KP_LCZ1.

In addition to profiler-type observations, we also used measurements of surface meteorological variables, including the 10- m wind speed (U10), 2-m temperature (T2), and 2-m relative humidity (RH2), retrieved from the Global Telecommunication System. The coordinates and LCZ classifications of these surface stations are provided in the supplementary material of Zhang (2024). The surface station dataset comprises a total of 13 urban stations characterized by LCZ classes 1 to 10, along with 10 stations situated on water *surfaces, and 8 rural stations on land. The distribution of surface stations across specific LCZ classes is provided in Table B1. '*

l.306 …the rural lidar station HT (first, I learn now that the different symbols are different sites (see previous comment), but also I learn that at least one of the lidars is 'rural'. Why not giving them an extension in the acronym?

We have renamed HT to HT_rural, USTSS to USTSS_LCZ5, and KP to KP_LCZ1 in the texts as well as in the figures.

l.306 at the LCZ 5 USTSS lidar location: wouldn't it be perfect to add this LCZ information to the section suggested in 'comment to Fig. 9'?

We have added the description of each LiDAR and its surrounding roughness in **Section 2.4.1**.

l.308 'has been reduced': the authors probably mean 'is smaller in the BEP schemes….'

Revised accordingly.

l.309 I don't think there is a Fig. A51… Can the authors adjust?

We have made necessary corrections with numbering when referencing to the supplementary materials.

l.312 starting at an altitude of 50 m agl?

The Vaisala wind LiDAR series WINDCUBE 100S has a blind spot from the ground level to 50m AGL. The first measurement starts from 50m AGL then every 25m.

Fig.10/11/12, captions: are these instantaneous values at the given times or 1-hour averages (in both, the observations and the simulation? Also, the caption may remind the reader that the panels start at 8 pm (why is this so?)

The LiDAR observations are 1-hour averaged values. The Vaisala wind LiDAR series WINDCUBE 100s is tuned to measure wind speeds at a frequency of 1 HZ. The size of raw data for a single LiDAR unit operating for 24 hours is on the order of 1 TB storage, thus the data is processed by taking the hourly average and save 24 times a day. The simulation is the instantaneous value defined at the integer hour in the WRF namelist.

The sequence of subplots starts from 8pm because the model integration starts from 1200 UTC+0 18th July in 2022 to 1200 UTC+0 18th August in 2022, which translates to 2000 local time in Hong Kong (UTC+8). Therefore, the default time stamp after aggregating the results starts from 20hr UTC+8, then 21, 22, 23, 24, 1, 2, 3, ...,18, 19. We have added the following sentence in the captions of Fig.9 and 10. for clarity.

'*The integration is from 2000 UTC+8 on* 18^{th} *July in 2022 to 2000 UTC+8 on* 18^{th} *August in 2022.*'

Fig. 13: RMSE and mean bias of WHAT? What is the data base? What are the 'error bars' referring to?

We meant the RMSE of monthly averaged diurnal variation of vertical profiles of wind speeds from WRF simulations by benchmarking against the LiDAR measurements, so is the mean bias.

The error bars mean the $\pm 1\sigma$ variability of RMSE/ mean bias calculated at 24 hours.

We have revised texts in the caption as:

'*Figure 13. RMSE (a) and MB (b) of the monthly averaged diurnal variation of vertical profiles of wind speeds calculated at the three LiDAR stations for four simulations obtained by taking LiDAR measurements as the ground truth. The error bars represent the* $\pm 1\sigma$ *variability of the RMSE/Mean bias of a diurnal cycle.*'

l.330 convective thermals

Revised accordingly.

l.331 the smallest RMSE and the smallest negative bias....

Revised accordingly.

l.332 Boulac+BEP, which increased the deviations with respect to the Boulac+bulk simulations.

Revised accordingly.

l.334 I cannot locate Section 44.1. please adjust.

We have removed the duplicated number and revised it as Section 4.1.

l.337 this is not predictability, rather 'accuracy'

We have revised the word 'predictability' as accuracy.

l.342 who is collaborating here with whom?

This is a typo where we meant 'corroborating'. We have revised the sentence in line 360 as:

‘*Both schemes coupled with BEP exhibited considerably decelerated wind speeds below ~ 400m, corroborating the trend observed for all LCZ 1 girds shown in Fig.C2a.*’

l.359 as small as...

Revised accordingly.

l.359 ‘...is more likely to be found at around 06LT in TKE-ACM2....’: I don’t think this can be said like that. Do the authors want to say that ‘delta_U10 starts to be larger (in absolute terms) starting from about 06 LT’?

We have overhauled Section 4.3 according to Reviewer#2’s suggestions, where we discarded the comparison between BEP and Bulk that is well-known and clear, rather we now present comparison between TKE-ACM2 and Boulac in BEP/Bulk simulations.

l.366 slightly altered?

Revised as ‘gently altered’ to ‘slightly altered’.

l.369 the ‘supplementary Zhang (2024) is not a proper citation (in the supplementary material to Zhang....)

We have revised line 387 as:

‘*Time series data for each station are provided in the supplementary material of Zhang (2024)*’

l.377 LC1...stations are ...lower than the observed values’: this is, first of all, not a correct sentence (the simulated wind speed at these stations is smaller than...). Second this is a very important observation, which suggests that the authors should (maybe in the appendix) produce a table where the LCZ codes are described in words (having read the sentence, I, for example would wonder what LCZ2 is (it is also having much lower wind speeds than observed....). I suggest to add this finding explicitly to the conclusions (in the present form it states that LCZ1,4, 10 etc. are underestimating – but it is more relevant to state that high-rise and heavy industry types are underestimating.

Thanks for your suggestion in highlighting our key findings. We have revised the text as:

‘*However, the simulated wind speeds simulated using BEP at LCZ 1, 2, 4, and 10 stations were lower than observed values, particularly during the day.*’

Second, we have added in the appendix outlining the code for each LCZ class along with brief description.

Third, we have highlighted explicitly that BEP simulations lead to underestimation in high-rise and industry type grids in the conclusion in line 446 to line 449:

'BEP did not necessarily improve the prediction of U10 at all types of urban stations as it could lead to largely underestimated U10 relative to the two schemes with Bulk methods. For instance, extremely low wind speeds were observed at LCZ 1, 2, 4, and 10 stations, which were in areas that had mostly compact or high-rise buildings. The enhanced accuracy of U10 simulated by TKE-ACM2+BEP was notable at stations located in areas of relatively low building density, such as LCZ 5, 6, and 8 stations.'

l.383 'at the hill whose.....': replace by 'at a hill with a spatial scale of 50 m'.

We have revised the wording in line 399 as:

'*For instance, the surface station co-located with the KP_LCZ1 LiDAR, also classified as an LCZ 1 station, was situated on a hill with a spatial scale of 50m*.'

l.388 'Coinciding with Fig. 15'? Maybe: 'As can be seen in Fig. 15, T2....'?

We have revised line 381 to '*Figure 15 shows that the temperature difference ΔT2(TKE-ACM2 – Boulac) followed a diurnal pattern, with TKE-ACM2 consistently simulating lower T2 at 12LT relative to Boulac which…*'.

l.391 'their predictability': it is accuracy and not predictability

The word 'predictability' is replaced by '*accuracy*'.

Figs17-19: what are 'G' stations?

We have added the code of LCZ class in the appendix as mentioned previously. In addition, we have replaced 'LCZ G' by explicitly referring to 'water surface'. We avoided frequent switching of wordings of 'LCZ G' and 'water surface' in the revised manuscript, instead, we used 'water surface' exclusively in the texts.

l.400 again, it is not the predictability that is improved, but the prediction (i.e., its accuracy). Predictability is a property of the atmosphere (which is assessed using ensemble prediction approaches)

Revised accordingly. The word 'predictability' has been revised in other places in the manuscript.

l.401 should read: ....BEP produces larger RH2 when ....

Line 420 is revised as:

'*Figure C5 shows that BEP produced an increasingly large RH2 when coupled with TKEACM2 rather than with Boulac, resulting in a more profound improvement in TKE-ACM2+BEP.*'

l.409 building-resolving

Revised accordingly.

Figure 20, caption: Please add the information (in the caption) where the number of sites contributing to a LCZ type can be found.

We have revised the caption of Figure 20 as:

‘*RMSE for aggregated station types, with a), b), and c) representing U10, T2, and RH2, respectively. The number of stations contributing to an LCZ type is given in the sub-titles in Fig.17, Fig.18, or Fig.19.*’

l.419 BEP suggests that the buildings act..

Revised accordingly.

l.421 ...observations are used to...

Revised accordingly.

l.425 ...LIDAR station, compared to..

The comma has been before ‘compared to’.

l.430 no predictability

The word ‘predictability’ has been replaced with ‘accuracy’.

References

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Response to Reviewer#2

The responses to the reviewers' comments are highlighted in blue, and the revised text is *italicized*.

Zhang et al., 2025: Coupling the TKE-ACM2 Planetary Boundary Layer Scheme with the Building Effect Parameterization Model

The authors present in the manuscript a development and performance of coupling of TKE-ACM2 PBL scheme with BEP urban model in WRF mesoscale model. Although it describes important and interesting topic of improving of WRF model performance, and also the design of the study seems reasonably, the manuscript is not well written. Sometimes it is hardly readable, confused, some parts are too long but other information are missing. The manuscript have to be substantially improved (or re-submitted) before publishing in GMD.

Specific major comments:

1/ The text of the manuscript is not well transparent, some results parts are too long, model formulation could be also shorter or moved into the appendix. Some short sections (e.g. 2.4) could be removed and the number of figures reduced. Further, the manuscript is hardly readable due to often quick switching between ideas and also missing links to figures. It seems that it was not preciously revised by authors before submission.

Dear Reviewer,

Thank you for your thoughtful and constructive feedback on our manuscript titled "*Coupling the TKE-ACM2 Planetary Boundary Layer Scheme with the Building Effect Parameterization Model*." We sincerely appreciate the time and effort you invested in reviewing our work. Your insights have been invaluable in guiding our revisions, and we have made significant changes to enhance the clarity and readability of the manuscript.

Model Formulation: In response to your suggestion, we have condensed the model formulation section by relocating some detailed derivations to the appendix. The numerical solutions to the prognostic equation (Eqn.3) are now removed from **Section 2.1 Numerical method to couple TKE-ACM2 and BEP** and instead detailed in Appendix A.

Removal of non-essential results discussion: Some non-essential parts of the text are removed to help focus on the key findings, e.g., the comparison between BEP and Bulk is removed as it is well investigated.

Removal of Short Sections: We have much shortened the introduction of the Local Climate Zones (LCZ) in the original **Section 2.4** without missing the essential information and reducing the reproducibility of the results.

Readability and Flow: To address the readability issues, we carefully revised the manuscript to ensure smoother transitions between ideas. For instance, we have revised in line 243 to line 305 where any confusion between the descriptions of Figures 4 and 5 is eliminated. The descriptions have been reorganized to clearly differentiate the two figures, ensuring that each is described in its own context without overlap. Meanwhile, we have included active references in Latex to relevant figures within the text when discussing the results. In addition, we have provided a Table B1 in Appendix B to demonstrate the number of available surface stations in each LCZ classification. We have also renamed the three LiDAR sites from USTSS, HT, and KP to USTSS_LCZ5, HT_rural, and KP_LCZ1, respectively according to Reviewer#1's suggestion to remind readers about the characterization of each LiDAR site and thus improve the readability.

Careful Revision: We apologize for any oversight in the initial submission and have conducted a thorough review to ensure the clarity and quality of the manuscript.

2/ Language level is not sufficient, proofreading by English native speaker would be appropriate.

Thank you for your constructive feedback regarding the language quality of the manuscript. We understand the importance of clear and effective communication in presenting our findings. In response to your comments, we have appointed a native English speaker from AsiaEdit (https://asiaedit.com/) to proofread and revise the manuscript. The track changes file shows the corrections to instances where language revision is needed.

3/ Description of model setting is insufficient, BEP parametrization setting of urban canopy parameters in specific LCZ is missing. Author does not consider possible inaccuracy in the setting of such parameters with impact to model performances in specific LCZ.

The BEP parameterization depends on an essential input known as the look-up table for urban morphology parameters (UCP) and thermal and radiative properties (URBPARM_LCZ.TBL when using LCZ). In this study, the look-up table remains as specified in the WRF 4.3.3 GitHub repository. Specifically, thermal properties such as emissivity, albedo, and thermal conductivity retain their default values. Similarly, the distribution of building heights for each LCZ adheres to the default generic values, which are detailed in Table B1 for clarity. These prescribed parameters are consistent with the values recommended by Stewart & Oke (2012).

The major limitation of applying the look-up table method for UCP is that the heterogeneity of UCP for a certain LCZ urban class is not considered, causing it less accurate compared to a gridded UCP approach (Sun et al., 2021). As reported by Shen et al. (2019), one of the crucial UCP, urban fraction, has paramount importance in simulating the horizontal wind speeds. However, the variability of urban fraction or building height distribution for a certain LCZ urban class is not taken into account in the present study.

In additoin, the process of re-gridding the LCZ global map from a 100-m resolution to a 1-km model cell raises concerns about the accuracy of the represented LCZ types (Ribeiro et al., 2021a; Sun et al., 2021). This challenge is further compounded by discrepancies between the land use at local observation stations and the land use depicted by the 1-kilometer model grid. Consequently, the UCP assigned to a specific LCZ type may lack adequate representativeness, especially when a model cell encompasses a variety of LCZ constituents, resulting in an absence of sub-grid variability.

We have added the abovementioned potential uncertainties in line 401.

4/ Description of LIDAR and station data is incomplete. Some special section about observation data is usual in papers, with information about measuring sites, variables, locations and other important characteristics in view of comparison with model data.

We have included an introduction to the LiDAR instrument in the revised **Section 2.4.1**, detailing its resolution, accuracy in measuring wind speed and direction, and operating frequency. Additionally, we have described the characteristics of the three sites where the LiDAR units are installed. Finally, we clarified that the classification of the measurement sites is based on the LCZ landuse associated with the nearest model grid following Ribeiro et al. (2021). Likewise, we have introduced that the surface station data is retrieved from Global Telecommunication System, where the method of classification of each station follows that of the LiDAR unit.

5/ Arrangement of Fig. 7, 8, 14, 15 and 16 shows rather impact of BEP urban scheme compared to Bulk, what is clear and well known fact, but not the impact of TKE-ACM2 PBL scheme compared to Boulac, which is the topic of the paper. Differences between simulations with/without TKE-ACM2 scheme should be rather displayed and also impact of TKE-ACM2 scheme more discussed.

We appreciate your guidance in helping us improve the alignment of our figures with the paper's core topic. We have made the following revisions according to your feedback: Figures 7, 8, 14, 15, and 16 have been updated to display the differences between the TKE-ACM2 and Boulac PBL schemes, both with and without the inclusion of the BEP urban scheme. Meanwhile, discussions in Section 4.1 and Section 4.3 are overhauled. This comparative analysis effectively highlights the impact of the TKE-ACM2 scheme, thereby reinforcing the focus and objectives of this study.

6/ High number of mistakes, typos, wrong use of dashes and connectors (see below). I would recommend to authors to use latex with active references for all figures, sections and tables, to enable better orientation in the text (showing of references by click on) and to prevent mistakes in numbering of figures, sections and tables.

Thanks for the careful evaluation. We have addressed all the identified mistakes in numbering and corrected the use of dashes and connectors. We also improved the phrasing and wording with the assistance from the native English proofreader. Additionally, we have ensured clickable references for all figures, sections, and tables are properly compiled in Latex. Detailed corrections can be found in the response below or in the track changes file.

Other comments and technical corrections:

L 12 – comparison to Bulk method, similarly L 27, that's not clear if Bulk is meant as some simple PBL scheme or simple urban scheme

Thanks for your comment. We have clarified the meaning of Bulk as 'without any urban scheme'. The sentence in line 10 has been revised to:

'*High-resolution wind speed LiDAR observations suggest that TKE-ACM2+BEP reduces overestimation in the lower part of the boundary layer compared with the Bulk method, which lacks an urban scheme, at a LiDAR site located in a densely built environment.*'

L 19 – brace near brace doesn't look well (L 32)

We have revised line 19 and line 33 as follows:

Line 19: … 'and the overlying roughness sub-layer, or RSL (Rotach, 1999).'

Line 32: 'The single-layer urban canopy model (SLUCM) pioneered by Kusaka et al. (2001); Kusaka and Kimura (2004) is …'

L 23 – 10-50

We have revised '10-50' to '10 to 50'.

L 35 – mathematical formula as Fi is superfluous in introduction (similarly L 41)

Agreed. We have removed the mathematical representation of multi-layer fluxes Fi in the introduction.

L 44 – what is urban heat island circulation? UHI or circulation in urban areas.

We have rephrased 'urban heat island circulation' to urban heat island effect' according to the cited work, i.e., Wang et al. (2017), stating that the urban heat island effect is well captured using BEP/BEM in Hong Kong.

L 48 – braces in braces doesn't look well (L 63 similarly, L 154)

We have revised line 69 as follows:

'*They showed that the TKE-ACM2 outperformed two other operational PBL schemes, Boulac (Bougeault and Lacarrere, 1989) and ACM2 (Pleim, 2007b), in simulating the vertical profiles of wind speeds.*'

We have revised line 154 as follows:

'*The prescribed height of building arrays is justified by that it is commonly seen in Hong Kong according to Kwok et al. (2020).*'

L 49 – word order … added recently by H…

Revised accordingly.

L 51 – motivation better explained

We have added a few sentences from line 48 to better emphasize our motivation in coupling a 1.5-order non-local closure PBL model with the BEP model:

'*However, multi-layer BEP/BEP+BEM models are adopted less widely than the Bulk scheme or SLUCM because they have only been tentatively coupled to a few planetary boundary layer (PBL) schemes [e.g., Boulac (Bougeault and Lacarrere, 1989), MYJ (Janjic, 1994), and YSU (Hong et al., 2006) added recently by Hendricks et al. (2020)]. This is primarily due to ´ the challenges associated with incorporating the transformation of mean kinetic energy into TKE within a first-order closure PBL scheme, such as the YSU scheme. As a result, the eddy diffusivity can only be adjusted in response to surface fluxes, limiting its ability to account for the generation and dissipation of TKE through other boundary layer processes, such as the generation of TKE by wind shear and buoyancy. Additionally, the other two PBL schemes (MYJ and Boulac) model the vertical mixing of momentum between two adjacent layers, but lack the non-local mixing driven by large-scale eddies under convective conditions. For instance, Coniglio et al. (2013) reported that MYJ produces PBLs that are too shallow and moist PBLs in the evening, and Xie et al. (2012) found that the PBL height diagnosed by Boulac may be too short to be realistic.*'

L 160 – the horizontal resolution of WRF+BEP in idealized case is not clear

The horizontal resolution of WRF+BEP was described in line 157:

'*WRF+BEP runs at ta building-parameterized scale ($\Delta x = \Delta y = 1$ km)*'

L 175 – WRF+BEP other setting is not described

The configuration of idealized WRF+BEP in Section 2.3 (line 149) is rather simplistic because the simulations are prescribed with idealized initial and boundary conditions, where physics such as microphysics and radiation scheme are turned off.

Specifically, the initial condition of wind speed is described in line 161 with the Coriolis parameter being $10^{-4}s^{-1}$ and that of potential temperature has analytical expression following Eqn.10 and Eqn.11. The landuse of all model cells is prescribed as urban type. The parameterization of cumulus and microphysics are turned off in WRF+BEP to keep it consistent with the LES setting. The short/long wave radiation schemes are also turned off because the net heat flux is prescribed with user-specified values. Additionally, the land surface model/surface layer schemes are not used for calculating surface fluxes for the same reason. However, the namelist options for these two physics (sf_surface_physics and sf_sfclay_physics) are still assigned with 8 and 1, respectively, otherwise BEP subroutines cannot be called.

An implication of this idealized configuration is that the thermal properties of buildings and streets specified in the look-up table (URBPARM.TBL) become ineffective because the radiation transfer is essentially prescribed by the idealized heat flux. The key parameters defined in the look-up table are a uniform building height of 40 meters, a street width of 30 meters, and a building width of 20 meters which are reported in the manuscript.

Chap. 2.4 – why is it separated? Is is used in idealized case, or is it belonging rather to real case?

We have reorganized Section 2.4.1 to describe the Local Climate Zones (LCZs) used in real case simulations. In addition, we have introduced the wind LiDAR observation network in this section, where we clarified the approach to classify the landuse type of the LiDAR unit.

L 186 – you talk firstly about 10 LCZ and here about 17 classes

The LCZ classification scheme has in total 17 classes, consisting of 10 urban classes and 7 non-urban classes. We have clarified in line 183 as follows:

'*The distribution of LCZ 1 to 10 (urban) grids and LCZ A to G 180 (non-urban) grids is depicted in Fig.2c. Each class is defined in Table B1*.'

Additionally, we have clarified the definition of 17-class LCZ in Appendix B Table B1 where the 10 urban classes along with the 7 non-urban classes are explicitly defined.

L 198 – the formulation "July 18 20 o'clock" is unclear, need reformulate. Similarly the following sentence.

Line 206 has been revised to:

‘*30-day simulations are performed between 1200 UTC+0 on 18th July to 1200 UTC+0 on 18th August of year 2022.*’

L 204 – this sentence is without any notice about moving to WRF setting

We have made the introduction to the physics settings of WRF a separate paragraph following the sentence “We used NCEP GFS analysis data at 6-hourly input intervals to provide the initial and lateral boundary conditions.”

The separate paragraph in line 213 reads,

‘*Identical physics schemes are chosen in the four simulations: unified Noah scheme (Chen and Dudhia, 2001) for the land-surface model, WSM 3-class simple ice scheme (Hong et al., 2004) for microphysics, RRTMG scheme (Iacono et al., 2008) for longwave/shortwave radiation, and Grell-Freitas ensemble scheme (Gall et al., 2013) for cumulus.*’

L 205 – Bulk scheme is usually not considered as a canopy model (UCM), because there is no canopy

Agreed. We have clarified that the Bulk scheme refers to the configuration where the surface layer fluxes are computed using Noah land-surface model without any UCM.

Lines 215 is revised to:

‘*The TKE-ACM2 PBL scheme was coupled with the BEP UCM (referred to as TKE-ACM2+BEP) and evaluated alongside the TKE-ACM2 scheme in isolation (TKE-ACM2+Bulk), where the surface layer fluxes were computed using the Noah land-surface model. The Boulac PBL scheme underwent the same evaluation, being coupled with the BEP UCM (Boulac+BEP) and assessed in isolation with the Noah land surface model (Boulac+Bulk).*’

L 215--220 – acronyms are unclear, all sentences should be written better

The mathematical symbols and acronyms are revised and the sentences are re-written in line 233 as:

‘*Quasi-equilibrium was achieved in the two LES cases after approximately 10.2 convective turnover times (τ), where $\tau = h/w^*$, and $w^* = \left(\beta \overline{w'\theta_0'} h\right)^{1/3}$ represents the convective velocity scale. The duration of 10.2 large-eddy turnover times is considered a reasonable indicator of well-developed dynamic fields over the domain with buildings, especially when compared to other studies that have used factors of 5 (Ayotte et al., 1996; Pleim, 2007b; Zhang et al., 2024) and 6 (Shin and Dudhia, 2016) for flat domains.*

The horizontal averages of the velocity and potential temperature fields are calculated at 10.2τ and serve as initial conditions for driving mesoscale WRF simulations for an additional 20τ . Subsequently, the results from the final 6τ, corresponding to either 3600 seconds or 2400 seconds, are averaged both horizontally and temporally. Table 1 summarizes the key turbulence characteristics of the convective flow and the runtime parameters.'

Fig. 4 – dotted line is not well visible in plots

Thanks for the comment. We have connected the dots representing the LES results with lines.

Fig. 5 – blue dotted and dashed lines are not in the legend

We have added the legend for the TKE-ACM2+BEP momentum flux which consists of the non-local (dashed) and the local (dotted) components in Fig.5.

L 240–250 and further – links to figures are missing, the text is still switching between description of Fig. 4 and 5

We have added necessary links and active references to figures from line 248 To line 309 to ensure that the references are clear and easily accessible for the reader. Additionally, we have revised the text to eliminate any confusion between the descriptions of Figures 4 and 5. The descriptions have been restructured to clearly distinguish between the two figures, ensuring that each figure is described in its own context without overlap.

L 256 – prorportion → proportion

Revised accordingly.

L 274 – is blue dashed line non-local or local component? (sentence vs. Fig. 5 caption), there is also no red dashed line

We have revised Fig.5 and also the texts so that the blue dashed line represents the non-local component and the blue dotted line denotes the local component.

L 275–276 – the sentence not clear

We have revised the sentence in line 295 as follows:

'Compared with Case 10WC, the larger prescribed $\overline{w'\theta'}_0$ in Case 24SC suggests that TKE-ACM2+BEP achieved a closer match in the magnitude and shape of $\overline{w'u'}$ at and immediately above roof level compared with Boulac+BEP. '

L 283 – fount → found

Revised accordingly.

L 283–285 – the sentence is not consistent to claim in L 244. I think a different order of variables in Fig. 4 and 5 vs. Fig. 6 caused it. I would recommend to change the order of variables in Fig. 6

Thanks for the comment. We have revised the order of variables in Fig.6 such that the order follows that in Fig.4 and 5, which reads $\theta, u, \overline{w'\theta'}, \overline{w'u'}$. The sentence in line 303 draws conclusions for Case 24SC where line 252 describes the results for Case 10WC.

To avoid confusion to readers, we have revised line 303 as:

'*This indicates that the two PBL schemes coupled with BEP performed similarly in simulating momentum profiles below the PBL height in Case 24SC and outperformed the Bulk methods.*'

L 289 – what does mean "other natural landuse" – is it any crop, forest or pasture?

We intended for 'other natural landuse' to refer to the landuse that is non-urban (LCZ 1 to 10) and also not water surface (LCZ G). To enhance clarity, we revised all instances of 'other natural landuse' to 'rural land cover' throughout the texts and figures.

L 299 – besides urban grid-boxes, the BEP model is not used over natural and water grid-boxes in simulation, so it cannot produce any direct difference in U, only as an impact of neighbouring grid-boxes

Agreed. We have rephrased the texts in line 311 as:

'*Both BEP simulations had less pronounced differences in U over water surfaces and rural land cover compared with urban grids, primarily because the BEP model was not directly applied in these non-urban areas. Any observed differences in U in these regions resulted from the neighboring urban grids.*'

L 302 – the sentence is not correct, there are other mechanisms except anthropogenic heat (e.g. shadowing of solar radiation by buildings), which cause lower temperature in BEP simulation in comparison to Bulk

Agreed. We realize that the total heat flux can consist of shortwave/longwave radiation received by the surface, and sensible heat through conduction computed in BEP, ultimately resulting in the lower temperature in our case. We have rephrased the sentence in line 319 as:

'*Finally, complex interactions between the atmosphere and buildings, including radiative transfer (direct and reflected solar radiation and net longwave radiation), and thermal exchange between solid surfaces and the atmosphere, collectively led to the lower temperature in BEP simulations.*'

Fig. 7 – there is no direct comparison of model and observation data

In response to your major comment #5, Figures 7 and 8 now focus on illustrating the impact of TKE-ACM2 on the vertical profiles of potential temperature and wind speed as compared to Boulac, both with and without the BEP. Additionally, the effects on 10-meter wind speed, 2-meter temperature, and 2-meter relative humidity are depicted in Figures 14, 15, and 16, respectively. This shift in focus better aligns with the core topic of our paper, moving away from the well-known comparison between BEP and Bulk methods.

We have also incorporated a comparison with observational data, including wind speed LiDAR measurements and surface station data, as detailed in Sections 4.2 and 4.3. Given the relatively sparse distribution of observation units in relation to the model grid, simulations are evaluated exclusively at grid points that encompass any measurement stations.

L 309 – there is no Fig. A51 in the manuscript

This sentence is deleted to avoid confusion.

L 331 – rather "the lowest RMSE and the lowest negative MB"

Revised accordingly.

L 335 – there is no Section 44.1

We have removed the duplicated number and revised as 'Section 4.1'.

L 369 – in the supplementary Zhang (2024) → rather in the supplementary material of Zhang (2024) … or similarly

We have revised it as '… in the supplement material of Zhang (2024)'.

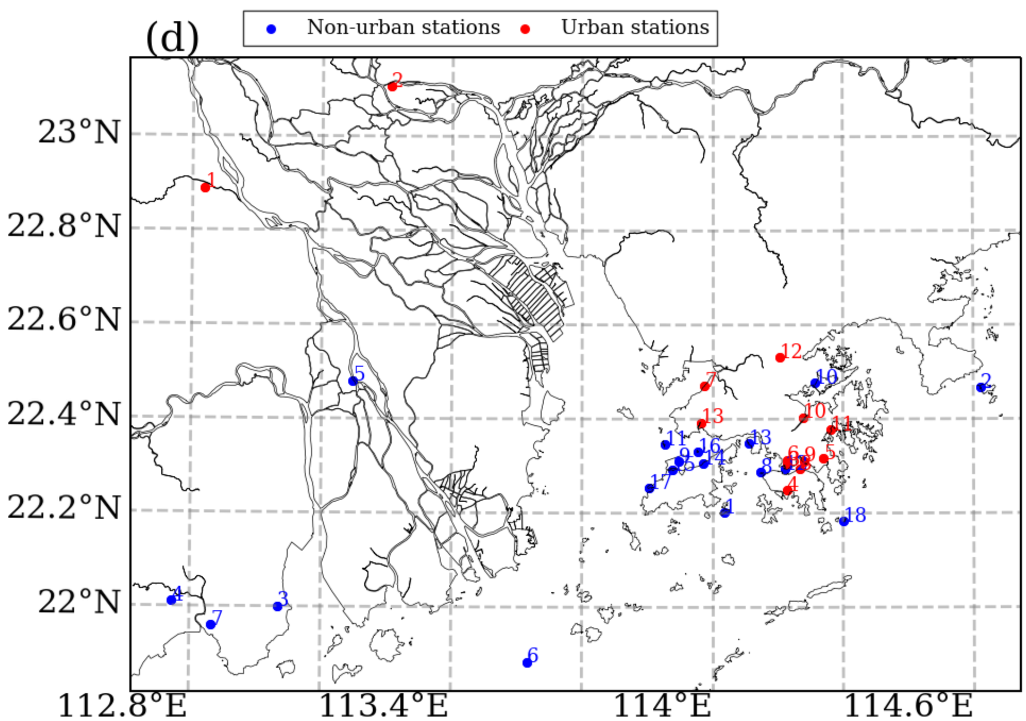
Fig. 17, 18 and 19 – it is not clear, how the stations are assigned to LCZ. Fig. 2 shows only 10 urban stations, but Fig. 17 etc. computes with 23 stations in urban areas.

The classification of each surface station is determined by the LCZ landuse of the nearest model cell center following Ribeiro et al. (2021). There are in total 13 urban stations (LCZ 1 to 10) and 18 non-urban stations (LCZ A to G) in the finest domain 4 (1 km resolution). The breakdown of all types of stations is listed below:

LCZ classification (urban)	Number of stations	LCZ classification (non-urban)	Number of stations
1 Compact high-rise	2	A Dense trees	4
2 Compact mid-rise	1	B Scattered trees	0
3 Compact low-rise	0	C Bush and scrub	3
4 Open high-rise	3	D Low plants	0
5 Open mid-rise	1	E Bare rock or paved	0
6 Open low-rise	2	F Bare soil or sand	1
7 Lightweight low-rise	0	G Water surface	10
8 Large low-rise	3		
9 Sparsely built	0		
10 Heavy industry	1		
Subtotal	**13**	**Subtotal**	**18**

The table above is included in Table B1 for clarity.

Figure 2d illustrates the distribution of surface stations, represented by blue and red circles. However, some stations overlap, making it difficult to assess their individual locations. To enhance clarity, the figure below focuses solely on the distribution of surface stations by removing the LiDAR units. This revised visualization clearly shows a total of 13 urban stations (red) and 18 non-urban stations (blue).



The following text has been added to clarify how the LiDAR unit and surface station is classified: '*We represent the land cover type of each LiDAR unit using the LCZ classification associated with the nearest model grid following Ribeiro et al. (2021).*' in line 186.

L 382 – reported in (Ribeiro et al., 2021) – wrong braces

We have revised line 399 as '*...reported by Ribeiro et al. (2021)*'.

L 400 – "influence of BEP is relatively marginal on RH 2 at non-urban stations" this is quite trivial meaning when BEP is not operating in non-urban grid-boxes.

Agreed. We have deleted the non-essential information.

L 419 – "BEP indicates the buildings act as a sink of heat" – I think this is not a correct statement, there is no sink of energy, the reasons for lower temperature under BEP are different.

Thanks for the careful evaluation. We are aware that the lower temperature simulated by BEP is a net effect from incoming and reflected shortwave radiation, received and outgoing longwave radiation, and conduction between the atmosphere and buildings. The phrasing of the sentence in line 437 has been changed to:

' *Likewise, the effects of BEP considering the radiative transfer and sensible heat fluxes between solid surfaces and the atmosphere ultimately led to a lower θ over all urban grids*.'

References

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